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Is there a desired added worker effect? Evidence from involuntary job losses

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Abstract

While the existing evidence on added worker effects is mixed, most studies find no or only small effects. However, studies to date have mostly analyzed individuals' actual labor supply responses to their partners' job loss, neglecting to consider a potential mismatch between desired and actual labor supply adjustments. Using data from the German Socio-Economic Panel (SOEP), we study individuals' changes in actual and desired working hours after their partners' involuntary job loss in an event study design. Our results show that actual and desired working hours only increase marginally and that these increases are of similar magnitude. Thus, we provide first evidence that the absence of more substantial added worker effects is in line with individuals' stated labor supply preferences and is not the result of an inability to realize desired working hours.

Keywords Labor supply · Desired working hours · Added worker effect · Event study.

JEL classification J22 · H55

1 Introduction

Against the background of increased labor market insecurity following recent global crises (COVID-19 and the Russian invasion of Ukraine) and major labor market transitions (e.g., the fourth industrial revolution and the transition to green energy),

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there has been renewed interest in the insurance function of the household when facing the consequences of a job loss. For affected households, an involuntary job loss represents a negative shock to their household income. In theory, this income shock can partially be mitigated by an expansion of the labor supply of the partner. In the economic literature, this potential increase in partner's labor supply is known as the "added worker effect" (Lundberg, 1985).¹ International evidence on added worker effects is mixed. While the bulk of economic studies on this topic finds no or only small added worker effects (e.g., Bredtmann, Otten and Rulff 2018; Cammeraat, Jongen & Koning, 2023; Halla, Schmieder & Weber, 2020; Hardoy & Schöne, 2014), just a few studies find substantial responses in partner's labor supply (Ayhan 2018; Schöne & Strøm, 2021). For Germany, the country analyzed in this study, recent studies find little evidence for a significant added worker effect (Fackler & Weigt, 2020; Illing, Schmieder & Trenkle, 2024).

Theoretically, there are at least two potential explanations for the absence of more substantial added worker effects. First, individuals might not want or need to markedly increase their labor supply in response to their partners' job loss. This can be for different reasons: For example, there might be a lack of incentive due to the generosity of the benefit system (Bredtmann et al., 2018; Cullen & Gruber, 2000) or there might be a lack of capacity or flexibility due to strong intra-household specialization (Bastani, Dickmanns, Giebe and Gürtler 2024). For instance, one partner might mainly focus on breadwinning while the other partner shifts their time to housework and care work. In such cases, it might be more cumbersome to change these roles within the household than it is for the partner who has lost their job to find a new one.

An alternative potential explanation for the absence of larger added worker effects, which so far has been vastly overlooked in the literature, is that while partners would in fact prefer to adjust their labor supply, they are not able to realize their preferred labor supply adjustment. At the intensive margin, for example, employees and their employers might not find a compromise regarding the extent of working hours adjustments. One can imagine a scenario where an employee might only want to increase their working hours from 20 hours per week to 25 hours per week, but the employer only offers a full-time position (40 h) or a part-time position (20 h). Furthermore, timing might be a factor: an individual might want to increase hours immediately after their partner's job loss, but the employer might need a considerable amount of time to restructure processes at work. By then, the partner could already be re-employed. Similar issues might arise at the extensive margin: an inactive partner might want to take up employment but might be unable to immediately find a suitable job due to a lack of experience or search frictions. Thus, while there may be a preference for an extension of labor supply, people may fail to realize it. However, since the vast majority of existing studies on the added worker effect at the intensive margin focuses primarily on changes in partners' actual working hours and earnings

¹ Added worker effects are also examined in other contexts, e.g. spousal labor supply responses to a partner's health shock (Beckmannshagen & Koenig, 2023; Bernasconi, Kantarci, van Soest & van Sonsbeek, 2024; Fadlon & Nielsen, 2021; Jolly & Theodoropoulos, 2023). However, like the majority of studies on added worker effects, we focus on spousal labor market responses to their partners job loss due to plant closures or dismissals.

(Bredtmann et al., 2018; Cammeraat et al., 2023; Fackler & Weigt, 2020; Halla et al., 2020; Illing et al. 2024; Stephens, 2002), they cannot capture potential changes in labor supply preferences. The idea that the response in observed working hours to a partner's job loss may not necessarily reflect the desired change in working hours was first proposed by Maloney (1987). However, to the best of our knowledge, no empirical study to date has extensively analyzed the effect of a partner's job loss on an individual's labor supply preferences at the intensive margin.² At the extensive margin of labor supply, a limited number of studies have analyzed the effect of partner's job loss on their spouse's job search behaviour, which can be interpreted as an increased preference for labor market entry (Bredtmann et al., 2018; Halla et al., 2020).

Using data from the German Socio-Economic Panel (SOEP, see Goebel et al., 2019) which offers information on both actual and desired labor supply, we examine reactions in actual and desired working hours to a partner's involuntary job loss. Thus, in addition to exploring actual added worker effects, we shed light on the question of whether a *desired* added worker effect exists. In doing so, we also analyze whether household income shocks constitute one root of mismatches between desired and actual working hours, a topic which has received more attention through the grown literature on labor market imperfections (Faberman, Mueller, Şahin & Topa, 2020; Manning, 2013).

We implement an event study analysis to compare the labor supply preferences of women whose male partners are affected by an involuntary job loss to those of women whose households do not experience such a shock to household income but are very similar in other individual and household characteristics. We consider involuntary job losses that occurred between 1999 and 2017. We specifically focus on women's labor market responses to their male partners' involuntary job losses because women are more often working part time and thus have a higher potential for an extension of labor supply.³ Accordingly, our main contribution lies in discovering whether or not there is a mismatch between women's responses in actual labor supply and their stated labor supply preferences after their male partners involuntarily lose their jobs. The primary focus of our analyses is the intensive margin of labor supply. Thus, our main analysis considers couples where the female partner is employed at the time of her partner's job loss.⁴ We also run an extensive set of

² Triebe (2015) touches on the effect of job loss on partners' stated preferences for an extension of labor supply in one of her sub-analyses. However, her analysis is very limited. As she compares desired working hours post-treatment with actual working hours pre-treatment, the analysis neglects the potential pre-existence of hours mismatches and therefore does not warrant a plausible interpretation of the effect of partners' job loss on desired working hours. Using data on Australian women, Gong (2011) shows that they are more likely to prefer an extension of their working hours after their partner experienced a job loss. However, the study does not quantify the magnitude of the desired increase in labor supply.

³ See Section 2 for a more extensive discussion. The full analysis is additionally conducted for men's (desired) labor market responses to their female partners' involuntary job loss. Results are presented in the Appendix. Due to a limited number of observations, we cannot include same-sex couples.

⁴ In a supplementary analysis presented in Fig. 9 in the Appendix, we also examine preferred and realized adjustments on the extensive margin, i.e. we analyse couples, where the female partner is not employed at the time of her partner's job loss.

sensitivity analyses to assess the robustness of our empirical strategy, investigate effect heterogeneity, and examine the reactions of strongly affected households.

We do not find evidence for a *desired* added worker effect that exceeds the size of actually realized hours adjustments. One period after the involuntary job loss of their partner, women increase their actual weekly working hours by one hour or 3% in relative terms. In the period thereafter the estimate is smaller and not statistically significant. This small adjustment in actual hours is largely in line with women's desired working hours. Desired working hours increase by about one hour in all post-shock periods. This finding is robust for several sub-groups and for different econometric specifications. However, in some situations—such as part-time working women or extended unemployment spells for men—we find indications of larger adjustments in actual and desired working hours. Furthermore, we provide insights into the labor supply dynamics after an involuntary job loss: the female partners' desired hours adjust immediately after the job loss while the increase in actual hours only occurs with a delay of one year. However, this mismatch resolves quickly. Overall, our results imply that the absence of larger added worker effects generally reflects the labor supply preferences of women and is not due to labor market frictions persistently preventing women from adjusting working hours in accordance to their changed preferences. Instead, we find small increases in both, women's stated labor supply preferences as well as their realized labor supply.

Our study contributes to two different strands of literature: first, the literature on the added worker effect and secondly, the literature on differences between desired and actual working hours. Following the seminal work by Lundberg (1985), a number of influential studies for different countries—e.g., Austria (Halla et al., 2020), the Netherlands (De Nardi, Fella, Knoef, Paz-Pardo & Van Ooijen, 2021), the U.S. (Cullen & Gruber, 2000) and a cross-country study for 28 European countries (Bredtmann et al., 2018)—analyze the added worker effect and suggest that the design of a country's unemployment insurance and benefit system shape the labor market reactions of spouses to their partners job loss.

For Germany, there are several studies analysing the added worker effect based on SOEP data. Despite finding substantial and persistent earnings losses of displaced workers, Fackler & Weigt (2020) find no evidence for a significant added worker effect when analysing partners' earnings after displacement. Triebe (2015) provides evidence for an added worker effect for married but not for unmarried couples. Another study based on SOEP data by Ehlert (2012) finds no evidence for a significant increase in working hours for West German women in response to their husbands' job loss. Illing et al. (2024) are the first authors to analyze the added worker effect using German administrative data. However, similar to earlier studies based on survey data, they find no evidence for an added worker effect in terms of earnings in Germany—regardless of the gender of the displaced worker. In fact, Illing et al. (2024) show that the opposite is the case: both for men and women, displacement leads to modest declines in their partners' earnings over the following years. The absence of a meaningful added worker effect for Germany is often explained by the generous tax and transfer system mitigating the income shock resulting from unemployment to such a large extent that partners' incentives to increase working hours are low (e.g. Ehlert, 2012; Fackler & Weigt, 2020).

By examining women's actual and desired working hours after their partner's job loss, we also contribute to a second strand of literature which focuses on the causes and consequences of mismatches between desired and actual working hours. Mismatches between desired and actual hours have severe consequences on individuals' health (Bassanini & Caroli, 2015; Bell, Otterbach & Sousa-Poza, 2012), well-being (Başlevent & Kirmanoğlu, 2014; Wooden, Warren & Drago, 2009), and the income distribution (Beckmannshagen & Schröder, 2022). While the consequences are wide-ranging, the causes of mismatch between desired and actual working hours are manifold: mismatches can arise due to a limited number of jobs available, job openings with non-negotiable working hours due to employers' higher bargaining power, search frictions or other market imperfections (see e.g., Altonji & Paxson 1992; Bloemen, 2008; Chetty, Friedman, Olsen & Pistaferri, 2011; Lachowska, Mas, Saggio & Woodbury, 2023).⁵ Adding to this strand of literature, one aim of this paper is to analyze whether shocks to household income can trigger hours mismatches related to labor market frictions.

The remainder of the paper is structured as follows: Section 2 describes our data source and the focal variables for the analyses. Section 3 explains the applied empirical strategy. Section 4 presents our main results as well as a set of sensitivity analyses. In Section 5 we discuss the results in the context of the existing literature while Section 6 concludes.

2 Data

2.1 SOEP data and focal variables

Our empirical analysis is based on data from the German Socio-Economic Panel (SOEP). The SOEP is a representative household panel survey which has been conducted on a yearly basis since 1984 and, as of 2019, comprises around 30,000 respondents living in 15,000 households (Goebel et al., 2019). The SOEP has a number of characteristics that are vital for our analysis. First, being the only available data source for Germany offering longitudinal information on both desired and actual working hours, the SOEP allows for a comprehensive investigation of a potential heterogeneous effect of involuntary job losses on partners' actual and desired working hours. Secondly, it offers a rich set of socio-economic information on individuals' labor market activities and preferences, allowing for detailed analysis and high-quality matching. Importantly, information is not only available for the household head but also for their partners. Therefore, due to its panel structure, the SOEP allows us to follow individuals and their partners over time, which is key for our analysis of actual working hours and working hours preferences before and after treatment.

The key variable to identify treated couples is the generated `pgjobend` variable which contains the reason why an individual's employment was terminated. The

⁵ For a detailed theoretical underpinning of the economic rationale behind desired working hours and mismatches between desired and actual working hours, see Beckmannshagen and Schröder (2022), Appendix A.

variable always refers to the time between the interviews of consecutive survey waves. Based on the `pgjobend` variable we compute a treatment indicator variable which categorizes people as treated if the given reason for an employment termination is either a dismissal by the employer or a plant closure. In line with the existing literature on causal effects of involuntary job losses (e.g. Chan & Huff Stevens, 2001; Hennecke & Pape, 2022; Kohara, 2010), we define both plant closures and dismissals as causes of involuntary job loss, which allows us to analyze the affected households' labor supply responses in a sufficiently large sample.⁶

In our main analysis we consider couples in which the male partner experienced a job loss due to dismissal or plant closure while at the same time the female partner was also employed. For these couples, our outcome variables are the following⁷:

- Men's employment status at the time of the interview
- Men's gross yearly labor income
- Women's actual working hours
- Women's desired working hours⁸
- Women's gross yearly labor income
- Household net yearly income
- Women's hours mismatch⁹

The first two outcome variables focus on the male partners' employment rate and income and thus capture how the men were affected by their involuntary job loss. The following four outcome variables aim at measuring the women's reaction and the effect on household net income. By examining women's working hours we focus on the intensive margin of labor supply of women already in employment. By contrast, in a supplementary analysis we consider couples in which the male partner experienced an involuntary job loss while at that time the female partner was not employed. For these couples our outcome variables are the following:

- Women's employment status at the time of the interview
- Women's intent to return to work¹⁰

⁶ Our results remain robust when we only analyze households affected by plant closures (see Fig. 4). Plant closures and mass layoffs are viewed as the most exogenous sources of job losses and are used in recent studies on the added worker effect drawing on large administrative data (e.g. Halla et al., 2020; Illing et al., 2024). However, the SOEP does not offer any information on mass layoff events and only focusing on plant closures would significantly reduce our sample size to 150 couples, which would not allow us to conduct sensitivity or sub-group analyses.

⁷ All outcome variables only refer to the primary job. The only exception is household net income, which also includes income from secondary jobs.

⁸ See Fig. 18 in the Appendix for the exact wording of the survey question capturing desired working hours. Also, see Beckmannshagen and Schröder (2022) Appendix B, where strong evidence for the validity, economic meaningfulness and the explanatory power of the desired working hours variable is provided. For example, it is shown that longer desired hours today predict an extension of actual working hours in the future.

⁹ The hours mismatch, Δh , is defined as the difference between desired and actual working hours.

¹⁰ The intent to return to work is surveyed with a 4-point Likert scale. For the analysis we recoded it as a binary variable, with "not at all" and "rather unlikely" recoded as 0, while "rather likely" and "for certain" recoded as 1.

By considering these variables, we capture intended or realized reactions on the extensive margin of employment, i.e., whether women who were not employed before their partners' job loss exhibit a higher tendency to take up employment after their partners' job loss. However, our sample comprises only a small number of couples, where the female partners were not employed before their partners' job loss. Also, labor supply adjustments on the extensive margin have already been analyzed based on larger administrative data bases (Halla et al., 2020; Illing et al., 2024). Therefore, the results for extensive margin outcomes are presented in the Appendix.

Our observation period covers the years 1997 to 2019, with job losses experienced between 1999 and 2017. Since we conduct an event study, it is important to be able to observe our key variables two years before and after treatment. The SOEP did not survey desired working hours in 1996, which is why the earliest event year that we study is 1999. We choose 2017 as our last event year because we do not want to analyze labor market outcomes that happened after the onset of the COVID-19 pandemic in 2020.¹¹

2.2 Treatment and control group

In our main analysis, we focus on exploring the effect of men's job loss on the actual and desired working hours of their female partners. Women are particularly affected by hours mismatches (Beckmannshagen & Schröder, 2022). Thus, exploring whether shocks to household income are a driving factor of women's hours mismatches is of great interest. Furthermore, the vast majority of male workers work full time (91%) with an average of 42 actual working hours per week. Thus, there is simply little scope for a labor supply extension among men. In contrast, only 48% of female workers are employed in full-time jobs and the average number of actual weekly working hours is 31 hours.¹² As a result, on average, there is more potential for women to extend their labor supply after their partners' job loss, making it particularly interesting to concentrate primarily on the labor supply adjustments of women. Nonetheless, we also conduct our analysis for the male partners' labor supply reaction to their female partners' job losses. The results are presented in Fig. 10 in the Appendix. As expected, we find no effects on men's desired and actual working hours after their partners' job loss.

For our empirical analysis, we divide couples into two groups: a treatment group and a control group. The treatment group refers to couples for whom the male partner is affected by involuntary job loss due to plant closure or dismissal. Couples in which the male partner was never affected by an involuntary job loss are assigned to the *unmatched* control group.¹³ Between the years 1999 and 2017, a total of 5082 men

¹¹ The pandemic and the regulatory reactions present a multifaceted shock to the labor market and, for example, due to closed childcare facilities, to the division of time and labor within households. Disentangling the different effects of an involuntary job loss and other pandemic-related measures is not feasible in our setting.

¹² The reported average full time rates and actual working hours for men and women are obtained from the unrestricted sample (time period 1999–2017) and refer only to individuals in regular employment.

¹³ Note that we do not impose further employment restrictions among the potential control group. This means potential control units have to be employed in the two pre-shock periods, but can drop out of employment voluntarily in the post-shock periods.

Table 1 Sample Restrictions and Observations

Sample restriction	Treated obs.	Control obs.
Men affected by involuntary job loss	5082	176,111
Employed in two years prior to job loss	3617	122,972
No civil servants or self-employed	3617	107,066
In stable relationship	1339	38,751
Age 24-55 at the time of job loss	1040	29,832
Both partners' employment status observed	1029	29,540
No missings in all matching variables	651	18,997
Only keeping the first experienced job loss	546	18,997
Outcome variables non-missing	470	17,046
After matching	449	449

Note: Table shows number of couples in treatment and control group after the stepwise implementation of sample restrictions. Note that the number of potential control observations (right column) refers to the number of potential controls before imposing the exact matching characteristics. *Source:* SOEP v37.

surveyed in the SOEP experienced a job loss due to plant closure or dismissal. However, as depicted by Table 1, imposing a variety of sample restrictions reduces the number of treated couples substantially. We only include men in stable employment relationships, meaning they were employed in the two years prior to their job loss ($t-2$ and $t-1$).¹⁴ Additionally, for reasons of comparability between the treatment and control group, we exclude civil servants and individuals in self-employment from the control group as they cannot be affected by plant closures or dismissals in the same way as regular employees. Furthermore, since our aim is to investigate the (desired) added worker effect within existing couples, we only consider couples in stable relationships. Our definition of stable relationships includes both married and cohabiting unmarried couples who remain together for the entire observation period from $t-2$ to $t+2$.¹⁵ Men affected by job loss have to be between the age of 25 and 55 in the year prior to treatment. We choose these age limits since we primarily want to analyze individuals who have finished their education and fully entered the work force while at the same time also excluding old-age workers to avoid transitions into early retirement. This age restriction also applies to their female partners. Some individuals face multiple involuntary job losses in their careers. In our main specification, we only consider their first observed job loss in the time period from 1999 to 2017 as we do not want to consider the same individuals twice at two

¹⁴ Following Illing et al. (2024), we include both men that were working on a full-time and men working on a part-time basis prior to their job loss in order to account for recent labor market trends of rising male part-time rates.

¹⁵ Although the SOEP data structure would theoretically allow us to follow both partners after a separation, we refrain from including couples separating during the five-year observation period. We do this in order to limit the risk that our investigations of labor supply responses to partners' job loss are biased by simultaneous labor supply adjustments around separation, which can be quite strong (see e.g., Brüggmann, 2020; Johnson & Skinner, 1986; Özcan & Breen, 2012). While we acknowledge the proven link between job loss and separation (see e.g., Charles and Stephens Jr., 2004; Eliason, 2012), its implication for our analysis is limited since we only exclude 34 couples which otherwise meet all requirements to be part of the treatment group.

different points in time. Also, we want to minimize the potential that couples have experienced similar shocks to their household income before and thus react differently due to anticipation.¹⁶ Our final pre-matching sample comprises of 470 couples in the treatment group¹⁷ and 17,046 potential couple-period observations in the control group.

By imposing these restrictions to both the treatment and control group, naturally both groups are at least to a certain extent comparable before job loss (see Table 2 in the Appendix). For example, they are of similar age and have a similar family situation (number of children, marital status). However, they also differ in a number of important characteristics on individual (e.g. tenure, gross labor income), establishment (firm size), and partner level (partners' working hours, household net income).

As our empirical approach relies on the comparison of couples in the treatment and control group to estimate the causal effect of involuntary job loss on partners' actual and desired labor supply, it is necessary to ascertain a high degree of comparability between both groups. We therefore apply a matching procedure to match treated couples to suited controls.

3 Empirical approach

We match control units from the pool of never-treated to treatment units based on a broad set of sociodemographic characteristics and labor market variables in the pre-shock period to obtain a control group that is closely comparable to our treatment group. More specifically, we match 1:1 and combine propensity score matching and exact matching. We match year-by-year and then stack treatment and control pairs from each year to ensure that we compare treatment and control group in the same year.¹⁸ Further, since the gender-specific labor supply decisions and gender norms in East Germany vary widely from West Germany (see e.g., Jessen, 2022), we only compare West (East) German couples with West (East) German couples. Lastly, while we also consider cohabiting unmarried couples if they are in a stable relationship, labor supply decisions of married couples might differ from those of unmarried couples, for example, due to joint taxation (Bick & Fuchs-Schündeln, 2017). We thus only compare (un)married couples with (un)married couples. Consequently, the exact matching variables are calendar year, marital status, and place of residence (East Germany or West Germany).

Propensity scores are estimated based on the following set of variables which refer to the male partners in the pre-shock period unless stated otherwise: age, education,

¹⁶ In a robustness check we instead consider only the latest involuntary job loss experienced in the observation period. As shown in Fig. 11 in the Appendix, this does not change our main results.

¹⁷ The involuntary job losses that we consider in our event study are fairly evenly distributed over time. An exception is the period from 2002 to 2005, which has particularly high numbers of involuntary job losses in our sample. During this period Germany was often referred to as the "sick man of Europe" with a recession in 2003 and high unemployment rates (Dustmann, Fitzenberger, Schönberg & Spitz-Oener, 2014).

¹⁸ Our approach is similar to Schmieder, Von Wachter and Heining (2023) and aims to avoid the Goodman-Bacon (2021) critique of event study designs with treatment in multiple periods.

gross labor income, gross labor income two years before job loss, tenure with firm, firm size, prior unemployment experience (measured in years), female partners' education, female partners' gross labor income, female partners' gross labor income two years before male partners' job loss, dummy variables indicating presence of children in the age groups under 3, 3 to 5, 6 to 12, 12 to 18, a dummy indicating a person in need of care in the household, household net income, household net income two years before the male partners' job loss, female partners' number of desired and actual working hours.

We select these variables because they likely influence household time use and labor supply. We only keep matched couples for whom common support is given.¹⁹ As shown in Table 2 the matching procedure results in 449 matched couples in the control group whose sociodemographic characteristics are very similar to the characteristics of the 449 treated couples with no statistically significant differences at the 95% significance level.

We analyze different outcomes of the matched groups in an event study framework. More specifically, we run OLS regressions with individual and year fixed effects as well as a set of dummies for the pre- and post-shock relative periods and their interactions with the treatment dummy. In this setting, the coefficients of the post-shock interaction dummies measure the period-specific average treatment effect of the involuntary job loss on the treated (ATT). The regression equation takes the form

$$Y_{it} = \sum_{k=-2, k \neq -1}^2 \gamma_k P_{it}^k + \sum_{k=-2, k \neq -1}^2 \delta_k P_{it}^k \times T_i + \nu_i + \tau_t + \epsilon_{it}, \quad (1)$$

with Y_{it} being the outcome of interest for person i in year t (e.g. a woman's desired working hours), $\{P_{it}^k\}_{k=-2, k \neq -1}^2$ being a set of relative period-dummies running from -2 to 2 but excluding the reference period $k = -1$, with a shock occurring between period $k = -1$ and period $k = 0$ if the person is in the treatment group. T_i is the respective treatment group dummy, ν_i are individual fixed effects, τ_t are year fixed effects, and ϵ_{it} is an idiosyncratic error. The coefficients of interest are the δ_k which can be interpreted as ATTs.

For our empirical strategy it is essential to assume that the involuntary job loss comes as a shock that is exogenous to the main outcome variables, i.e. the female partner's labor supply preferences.²⁰ Thus, the central identifying assumption for allowing a causal interpretation of the coefficients is the common trend assumption (see e.g., Goodman-Bacon, 2021). In our application the common trend assumption states that in the absence of the male partner's involuntary job loss, differences in outcomes of the treatment and control groups would remain constant. If this assumption holds, one can interpret the differences in outcomes between treatment

¹⁹ We use the Stata command `kmatch` (Jann, 2017) and specify the `comsup` option, which ensures that common support is given based on the minima and maxima comparison (Caliendo & Kopeinig, 2008).

²⁰ In the context of the partner's labor supply preferences, it is very likely that our outcome of interest—desired working hours of female partners—is uncorrelated to their male partner's dismissal. We therefore consider both job loss due to plant closure and due to dismissal as an exogenous shock to household income. In a robustness check we consider plant closures and dismissals separately and the outcomes do not significantly differ, which strengthens our joint approach (see Fig. 4).

and control group, i.e. the depicted coefficients, as causal effects. As support for the common trend assumption, we depict pre-trends, i.e., differences for all outcome variables, prior to the treatment in our result graphs. To account for potential heterogeneous treatment effects over time, we conduct a robustness check where we re-run our analyses using the estimator proposed by Callaway and Sant'Anna (2021).

4 Results

The presentation of our results is structured in three parts. First, we show our main analyses that focus on women's intensive margin labor supply adjustments after their partners experienced a job loss. Thus, in these analyses we consider all women who are in employment when their partners' job loss occurred. Secondly, we concentrate on women who work less than full time before their partners' job loss. Naturally, the potential scope for the added worker effect is larger among these women, which is why the distinction between full-time and part-time working women is of particular interest. Lastly, we conduct a comprehensive set of tests to assess the robustness of our results.

4.1 Main analysis of intensive margin adjustments

Figure 1 comprises six panels with the outcome variables of interest. Panel A and B show how the male partner was affected by a job loss. Panel A depicts the employment rate of male partners. By construction, all male partners in the treatment and control group were in employment in the two years prior to the job loss. While all men in the treatment group experience a job loss between period -1 and 0, the employment rate of treated men drops by 45 percentage points (pp) compared to the control group in period 0. Thereafter, there is a partial recovery and the effect is around -25 pp in period 1 and 2. Accordingly, many of the men affected by a job loss find a new employment relatively quickly.

Panel B shows how men's yearly gross labor income is affected by a job loss. In period 0, it drops by around 9000€ compared to the labor income of the control group. Thus, the unexpected job loss clearly affects men's labor income with an average relative reduction of about 29% in period 0. Again, one can observe a slight recovery in the periods thereafter: in period 2 the effect is at around -7500€. However, as depicted by Panel A, a substantial proportion of the affected men take up a new position relatively quickly and are back in employment at the time of the survey in period 0, which is also reflected in their incomes.

Panel C shows how the female partners' actual working hours react to the job loss of their male partners. There is no immediate reaction to the job loss in period 0. In period 1, the point estimate indicates a slight increase in actual working hours of one hour, which is also statistically significant. In period 2, this estimate is slightly lower and not statistically significant at the 95% level. Consequently, we do not find an immediate adjustment on the intensive margin of labor supply but a very small added worker effect of one hour or 3% one year after the partners' involuntary job loss.

At the same time, the stated labor supply preferences of women might still have changed to a different extent after their male partners' job loss. Panel D therefore

depicts how women's desired working hours develop after the shock to household income. For all post-shock periods, the point estimates indicate an increase in desired hours by around one hour. The respective estimates are on the verge of statistical significance in each of the three post-shock periods.

In Panel E we examine women's labor income after their partners' job loss. Similarly to women's labor supply, the point estimates indicate a small increase in women's labor income in period 1 and 2. However, none of the estimates is statistically significant.

Lastly, in Panel F, we analyze how the men's involuntary job loss affects household net income. In the two periods after the shock, household net income drops on average by about 3000€, which amounts to a decline of 6% relative to the pre-shock household net income. Thus, the effect of the male partners' job loss on household net income is considerably smaller (both in absolute and relative terms) than its effect on men's individual gross labor income (Panel B). Furthermore, in general, household net income seems to recover more quickly after men's job loss. In period 2, the point estimate is reduced by one third and only at -2000€. Both the small magnitude and the indicated short-term nature of the drop in household net income might explain the absence of larger adjustments in the female partners' labor supply or labor supply preferences.

The depicted event study figures are informative regarding the period-specific dynamics after an involuntary job loss. We complement the period-specific analyses with a more standard difference-in-difference estimation comparing all post- to all pre-job loss periods. The results are displayed Table 3 in the Appendix. They generally reinforce the results of our main analysis: Small effects on women's actual and desired working hours of similar magnitude.

As mismatches between desired and actual hours can have serious consequences on earnings and well-being, we are particularly interested in whether a shock to household income due to a partner's job loss triggers such mismatches. Therefore, in Fig. 2 the effect of male partners' job loss on women's hours mismatches is examined. Similar to the findings for desired and actual working hours, there is no clear evidence for a substantial change in hours mismatches after the male partners' job loss. While the point estimates indicate a minor increase in the mismatch after the shock, this quickly dissolves and none of the coefficients is statistically significant at the 95% confidence level. Thus, there is no indication that a partner's job loss can be considered as one of the drivers of women's hours mismatches.

4.2 Focusing on part-time working women

A fair criticism of our main analysis might be that about 50% of the women in couples affected by a job loss are already working full time²¹ and therefore have only little opportunity to extend their working hours. It is true that while the legal maximum in Germany is at 48 hours per week, most full-time jobs have contractual working hours of around 40 hours and are not easily extendable above that number.

²¹ This number is based on the women's self-assessed employment status, not on a strict hours cut-off between full-time and part-time employment.

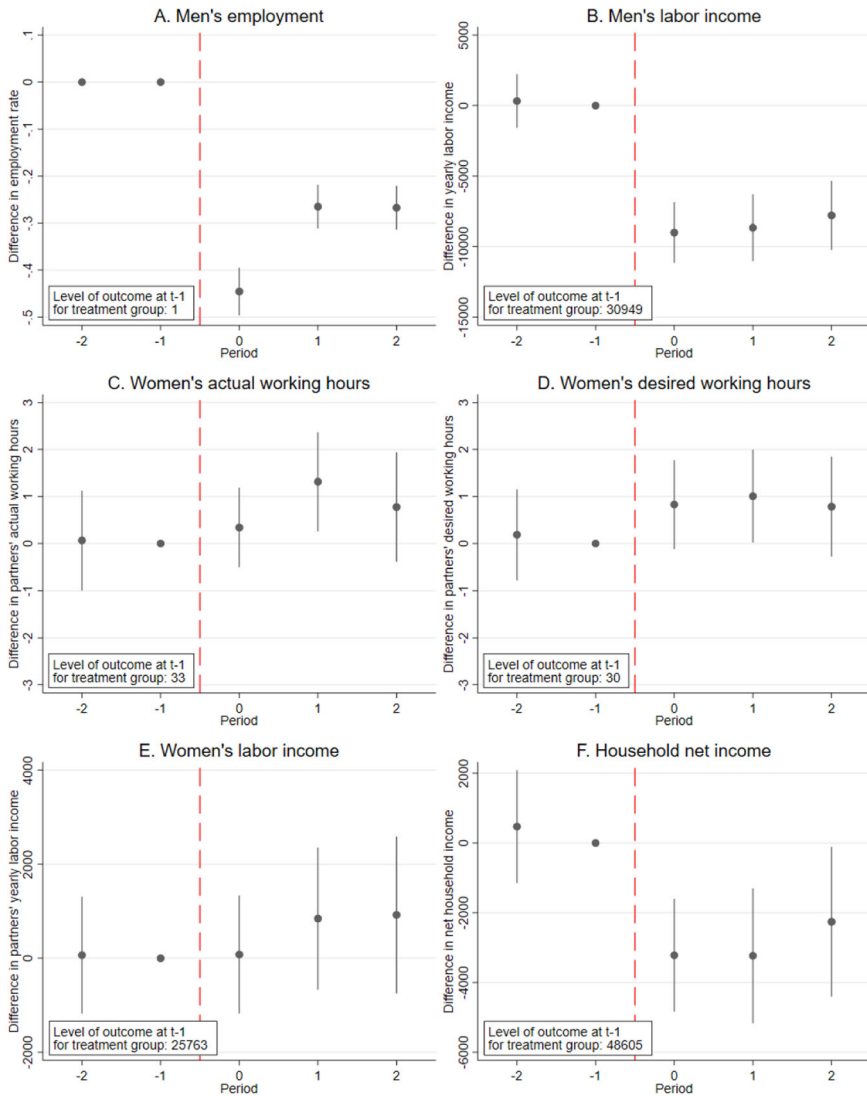


Fig. 1 Main outcomes after men's job loss. *Note:* Shows period-specific coefficients according to Eq. (1). Bars give robust 95% confidence intervals of the respective coefficients. All incomes are price-adjusted and presented in 2019 Euros. Number of individual observations: 898 (449 treated, 449 control units). *Source:* SOEP v37.

Thus, the potential scope for the added worker effect is naturally larger among women working part-time jobs than among women working full-time jobs.

For the following sub-analysis we therefore restrict our sample to comprise only couples in which the female partner works a maximum of 35 hours per week prior to her partner's job loss. This reduces the analyzed sample by 60% and thus inevitably comes with a loss in precision which is why our interpretation will only focus on point estimates. Figure 3 shows the results for actual and desired working hours in

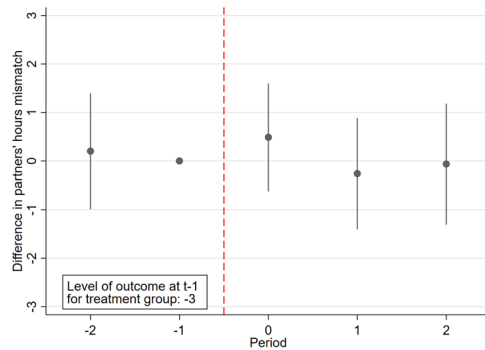


Fig. 2 Women's hours mismatch after men's job loss. *Note:* Shows period-specific coefficients according to Eq. (1). Bars give robust 95% confidence intervals of the respective coefficients. Number of individual observations: 898 (449 treated, 449 control units). *Source:* SOEP v37.

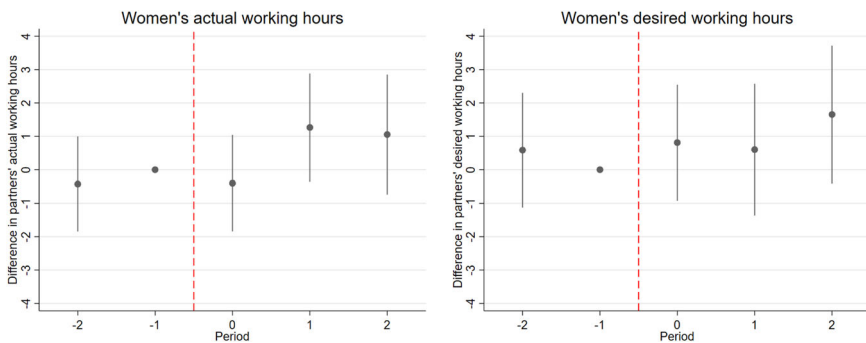


Fig. 3 Women working 35 hours or less before their partner's job loss. *Note:* Shows period-specific coefficients according to Eq. (1) for a sample restricted to women who work a maximum of 35 weekly hours in period -1. Bars give robust 95% confidence intervals of the respective coefficients. Number of individual observations: 360 (180 treated, 180 control units). *Source:* SOEP v37.

this restricted group. Generally, the results are qualitatively very similar to those of the main analysis. The point estimates indicate an increase in weekly actual working hours of just above one hour in period 1 and 2. For desired hours an increase of similar magnitude already occurs in period 0. However, in period 2, the point estimate for desired hours is closer to two hours. Overall, even women who were working 35 hours or less before their partner's job loss are not responding with a substantially larger extension of actual or desired working hours compared to the full sample. Only in period 2, there is some indication for slightly higher desired hours among these part-time working women.

4.3 Robustness checks

We conduct a variety of sensitivity analyses to assess the robustness of the estimated effects and examine whether the results change for some particularly affected couples. The sensitivity analyses can be classified into three broad categories. First, we

investigate the robustness of our empirical strategy by (a) using an alternative matching approach, (b) an alternative estimator proposed by Callaway and Sant'Anna (2021), (c) examining potential anticipatory effects and (d) differentiating between different types of involuntary job loss (plant closure vs. dismissal). Secondly, we explore the heterogeneity of women's (desired) labor market responses to their partners' job loss for a number of different sub-groups (e.g. women with vs. without children, married vs. unmarried couples). Lastly, we investigate whether shock *intensity* (e.g. duration of men's unemployment, relative loss in household income) matters for women's (desired) working hours adjustments following their partners' involuntary job loss.

4.3.1 Robustness of empirical strategy

First, we provide an alternative to our baseline estimates, which are based on exact matching and propensity score matching. Figure 12 in the Appendix presents estimates based on exact matching and the commonly used Mahalanobis distance matching approach (Mahalanobis 1936) using the same matching variables. The results based on this alternative matching approach are fairly similar to our baseline estimates with only small increases in both working hours variables that are not statistically significant in most post-shock periods. At the same time, some of the pre-shock estimates suggest that the Mahalanobis distance matching in this case is inferior to our chosen matching strategy in the main analysis in terms of balancing the characteristics of our treatment and control groups.

Furthermore, the recent literature on dynamic treatment effects has emphasized the importance of taking into account time-specific heterogeneous treatment effects when estimating ATTs in standard two-way fixed effects models and pooling events at different points in time (Goodman-Bacon, 2021). To ensure that heterogeneous treatment effects along event timing do not bias our main estimates, we re-run our main analysis using the estimator proposed by Callaway and Sant'Anna (2021). The aggregate group-time ATTs estimated based on Callaway and Sant'Anna (2021) do not substantially alter our results (see Fig. 13 in the Appendix). This is not surprising given the fact that we follow Schmieder et al. (2023) in their strategy of stacking matched treatment-control groups for each year of job loss, which already has strong similarities to the estimator proposed by Callaway and Sant'Anna (2021). The only notable difference using the Callaway and Sant'Anna (2021) estimator is the pre-shock estimate in the analysis of actual working hours. The positive point estimate of 0.8 hints at some anticipatory behavior but is not statistically significant.

A related issue that could potentially threaten our identification strategy is that treated employees might anticipate a plant closure or their dismissal. In this case, the involuntary job loss may not come as a shock and their female partner could proactively react by adjusting their labor supply before the involuntary job loss occurs (Hendren, 2017). Consequently, the proactive adjustments of preferred and actual labor supply would lead to no further effects after the involuntary job loss and explain the absence of more substantial effects in our main analysis. While we cannot directly observe whether affected employees anticipate their job loss, we are able to assess whether there is any indication that their partners adjust their actual and desired labor supply in expectation of their partner's job loss. To rule out any

anticipatory effects in women's actual or desired working hours, we shift the observation window to include four periods before the involuntary job loss. Figure 14 in the Appendix shows that there are no visible adjustments in actual hours in the years before job loss, as all points estimates are very close to zero. The point estimates for women's desired hours indicate slightly higher desired hours three and two years before their partners' job loss. However none of the coefficients is statistically significant and the point estimate in the period right before their partners' job loss is zero. Thus, we find no evidence that the absence of larger effects in actual and preferred labor supply is a result of anticipatory reactions before the job loss occurs.

Additionally, we differentiate between two different types of involuntary job loss: layoff due to plant closure and dismissal. There is an extensive literature on the effects of unexpected job loss focusing solely on individuals affected by plant closures or mass layoffs, as these job loss events are seen as credibly exogenous (e.g., Marcus, 2013; Schmieder et al., 2023). However, as already explained in Section 2.1, we also include dismissals as another type of involuntary job loss. In order to make sure that also including dismissals does not bias our main results, Fig. 4 shows outcomes after differentiating between couples for which the male partner was dismissed and couples for which the male partner was affected by plant closure. As we can see in Fig. 4 (Panel A), the drop in the employment rate between period -1 and period 0 is more severe for dismissed workers (-50 pp) than for individuals who lost their job due to plant closure (-32 pp). This large drop in the employment rate is accompanied by an income loss of around 10,000€ for dismissed workers. At the same time, income loss of men affected by plant closure between period -1 and period 0 is around 5500€. While employment rates converge very quickly, differences in income losses seem to be more persistent for these two groups. The female partners' responses in labor supply (preferences) are difficult to assess due to the loss of precision after splitting the sample. Interpreting the point estimates indicate a slightly higher response in actual hours among women whose partners were affected by a plant closure. In contrast, for desired working hours, the point estimates for women with dismissed partners are higher, which could be the due to the persistently higher income losses that these couples experience. However, most importantly, no significant differences in actual and desired labor supply responses can be found for these two types of involuntary job losses.

4.3.2 Heterogeneity by sub-groups

In our baseline analysis, which is based on the full sample, we only find small female labor supply responses to male partners' involuntary job loss—both with regards to their actual and desired working hours. In order to gain a deeper understanding of whether this small response is universal or not, we perform a number of different sensitivity checks to analyze heterogeneity in labor supply responses for different sub-groups.

First, we investigate whether married women react differently than unmarried women. In our main specification we consider all cohabiting couples in stable relationships, regardless of their marital status. However, theoretically, there are different ways in which marital status could influence labor supply responses. For

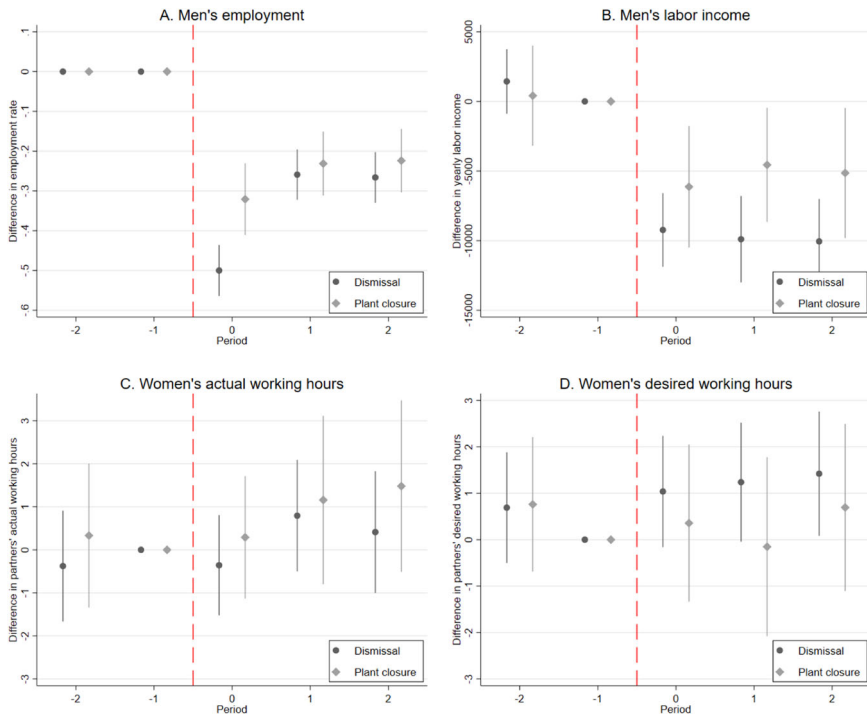


Fig. 4 Different types of involuntary job loss—dismissals vs. plant closures. *Note:* Shows period-specific coefficients according to Eq. (1) differentiated for couples affected by dismissal or plant closure. Bars give robust 95% confidence intervals of the respective coefficients. Gross labor income is price-adjusted and presented in 2019 Euros. Number of individual observations: Plant closure: 268 (134 treated, 134 control units); dismissal: 556 (278 treated, 278 control units). Note that the overall number is lower than in our main specification because the underlying logit estimations of our year-by-year propensity score matching procedure do not converge in all years due to the small sample size after splitting the sample. *Source:* SOEP v37.

example, unmarried women might not respond as strongly to their partners' job loss as married women since they might not see themselves as forming an economic unit with their partner. Indeed, evidence of a lower degree of income pooling in unmarried compared to married couples (e.g., Evans & Gray, 2021; Hiekel, Liefbroer & Poortman, 2014) can be interpreted as a sign of higher degrees of individualism and independence. In line with this argument, Triebe (2015) finds evidence for an added worker effect for married but not for unmarried couples. On the other hand, higher marginal effective and participation tax rates due to joint taxation of married couples could prevent wives from increasing their labor supply at the extensive and intensive margin—especially once their partners are re-employed (see e.g. Bick & Fuchs-Schündeln, 2017). To assess whether marital status impacts our results, we exclude unmarried couples from the analysis, which reduces our sample size by 15%. As depicted by Fig. 15 in the Appendix, no substantial differences to our baseline results occur when only analysing married couples.

Next, we examine the role of children in the household for the effect of partners' job loss on female labor supply. Raising children is one of the major determinants of

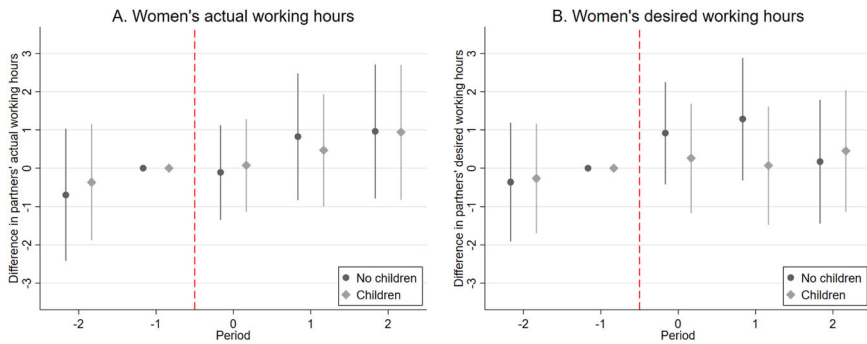


Fig. 5 Results for households with children vs. households without children. *Note:* Shows period-specific coefficients according to Eq. (1). Bars give robust 95% confidence intervals of the respective coefficients. Number of individual observations: No children: 346 (173 treated, 173 control units), children: 440 (220 treated, 220 control units). Note that the overall number is lower than in our main specification because the underlying logit estimations of our year-by-year propensity score matching procedure do not converge in all years due to the small sample size after splitting the sample. *Source:* SOEP v37.

within-household division of time and thus also of labor supply decisions. Accordingly, the presence of children in different age groups has major implications for the added worker effect (Cammeraat et al., 2023; Halla et al., 2020). Therefore, in Fig. 5, we differentiate between the labor supply responses of women with children and women without children living in the same household. Again, splitting the sample comes at the costs of losing precision which is why we focus on the interpretation of point estimates. For actual working hours, the differences between women with children and women without children are minor. The latter seem to adjust their actual labor supply slightly faster than women with children. For desired working hours the differences are more pronounced. Desired working hours of women without children seem to increase substantially stronger than desired working hours of women with children. Thus, while the differences are not statistically significant, we find some indication that labor supply preferences increase more immediately and to a larger extent for women without children than for women with children.

4.3.3 Accounting for shock intensity

One of the potential reasons for the absence of stronger labor supply responses could be the limited intensity and persistence of the income shock that households experience after the male partner's job loss. As depicted in Panel A of Fig. 1 and described in Subsection 4.1, the majority of men are back in employment relatively quickly and average effects on household net income are modest.²²

The following set of sub-analyses examines whether labor supply reactions of female partners differ for couples for whom the experienced shock was particularly severe. First, we repeat our main analyses but restrict our sample to only include couples in which the male partner was still out of employment when the interview in period 0 was conducted. Figure 6 shows results for this sub-group analysis.

²² The average effect in period 0 is at 6.6% (−3218€ relative to the baseline of 48,605€), while the average effect in period 2 is at 4.6% (−2259€ relative to the baseline of 48,605€).

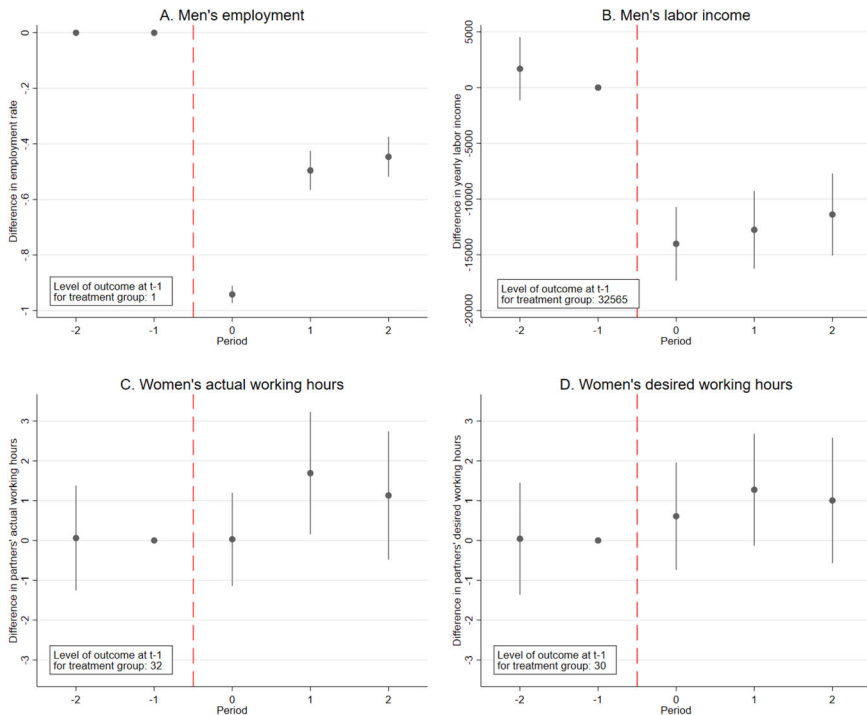


Fig. 6 Larger shock intensity: longer unemployment spells. *Note:* Shows period-specific coefficients according to Eq. (1) for a restricted sample of only couples in which the men were still unemployed in period 0. Gross labor income is price-adjusted and presented in 2019 Euros. Bars give robust 95% confidence intervals of the respective coefficients. Number of individual observations: 448 (224 treated, 224 control units). *Source:* SOEP v37.

Panels A and B show that—as expected—the shock was more intense and more persistent for this group. For men affected by an involuntary job loss, the employment probability in period 0 is reduced by 95%²³ while yearly gross income on average drops by almost 15,000€, which amounts to more than 40% of pre-shock income. Thus, the negative income shock is substantially larger than it is in our baseline analysis where yearly gross income on average drops by around 9000€ (see Panel B of Fig. 1). Panel C displays actual hours of the female partner. As for the main analysis, we do not find an immediate effect on actual hours. However, in period 1, actual hours of women whose partners were affected by an involuntary job loss increase by about 1.8 hours per week. In period 2, the point estimate indicates an attenuation of the effect to about one hour. Due to a lack of precision after reducing the sample by more than half, the estimate in period 2 is not statistically significant. Panel D shows the female partners' desired working hours. In all post-shock periods point estimates are positive, indicating a slight increase of one hour on average. Again, the reduced sample size leads to a loss of precision

²³ Note that the ATT is not 100% because some men in the control group might also move out of employment for other reasons than an involuntary job loss.

which is why none of the point estimates for desired hours are statistically significant.

Secondly, we take into account the relative income loss that couples experience due to the involuntary job loss. We restrict the sample to contain only couples in the top half of relative income loss from period -1 to 0. Results are presented in Fig. 16 in the Appendix. The results are generally very similar to the results of the sub-group analysis for couples in which the male partner was still unemployed in period 0. Point estimates for both actual and desired hours indicate increases in post-shock periods that are slightly larger than in the main analysis but are insignificant in most periods.

Lastly, we restrict the sample to contain only couples, in which the male partner worked in full-time employment prior to losing his job. As 95% of men in the treatment group work in full-time employment, this only slightly reduces the sample. Due to the large overlap with the baseline sample, the results which are displayed in Fig. 17 in the Appendix mirror our main results, as would be expected.

Overall, examining sub-groups for whom the experienced shock was particularly severe yields insights with respect to the dynamics of female labor supply preferences after the male partners' job loss. There is evidence that women's actual and desired working hours tend to increase to a larger extent after a job loss that puts their male partners out of employment for a longer period of time. However, due to the small sample size these results should be interpreted rather cautiously.

5 Discussion

The main focus of our study is to analyze both the actual and the desired labor supply response of women to their male partners' involuntary job loss. Primarily, we investigate changes at the intensive margin, i.e. changes in actual and desired working hours. Our results indicate a small increase in actual working hours of around one hour. However, the effect is only statistically significant in the year after the partners' job loss occurred. Moreover, we provide novel evidence that this small increase in actual working hours is in line with changes in desired working hours. We therefore show that the absence of a more substantial added worker effect generally reflects women's stated labor supply preferences and cannot be attributed to labor market frictions that prevent women from adjusting their working hours according to their changed preferences. However, we find some evidence suggesting that women's stated working hour preferences react immediately (period 0) after their partners' job loss, while actual changes in working hours occur slightly later (period 1).

At the same time, according to our results, the small increase in working hours does not translate into a significant increase in women's labor income. Thus, in line with existing studies for Germany (e.g., Fackler & Weigt, 2020; Illing et al., 2024), we find no evidence for a significant added worker effect in terms of income gains.

Several potential reasons why women do not seem to desire a larger increase of their labor supply are conceivable. One reason could be that the loss in household net

income is not that severe or sufficiently long-lasting for women to adjust their labor supply preferences. In line with recent studies for Germany (e.g., Illing et al., 2024; Jarosch, 2023; Schmieder et al., 2023) we find evidence for, on average, significant and persistent gross earnings losses for individuals affected by job loss (see Fig. 1, Panel B). However, reduction in household net income (after taxes and benefits) is much less severe (see Fig. 1, Panel F). Thus, the German tax and transfer system plays a substantial role in mitigating the income shock, which is in line with findings by Ehler (2012) and Fackler and Weigt (2020).²⁴ Fackler and Weigt (2020) show that redistributive measures of the tax and transfer system reduce the household income gap between couples affected by job loss and their non-affected counterparts by around 93% in the first year after job loss and by approximately 72% in the longer run (five years after job loss occurs).

In addition to the rather generous unemployment insurance system, many of the male partners affected by an involuntary job loss appear to find new jobs relatively quickly. In fact, the effect on men's employment implies that the employment rate in the treatment group is only reduced by 25% compared to the control group in period 1 (see Fig. 1, Panel A). Thus, for the majority of the affected men, the time between job loss and re-employment appears rather short.²⁵ As a result, it is likely that in most cases the male partners' temporary unemployment does not lead to a persistent change in the intra-household division of housework. While Foster and Stratton (2018) show that significant labor market events can indeed affect the division of time spent on housework between partners, recent results for Germany by Hennecke and Pape (2022) cast doubt on the persistence of these effects. Hennecke and Pape (2022) show that a father's job loss significantly increases paternal childcare and housework in the short-run; however, effects reverse shortly after re-employment and no strong evidence for persistent changes in bargaining power or gender role attitudes exists. Against this background, the lack of a more substantial response in women's actual and desired working hours could also be the result of both factors: first, the anticipation that short-term changes in the intra-household division of housework and childcare are not persistent and second, the actual reversal to the prior intra-household division of these tasks once their partner finds a new job. Generally, gender roles in Germany are on average more traditional than in most other Western societies (Kleven, Landais, Posch, Steinhauer & Zweimüller, 2019). These traditional gender roles in Germany might be a crucial determinant for the absence of a larger labor supply response by women, as they on average carry out most household and

²⁴ For example, under the current regulations, former employees who were employed for at least 12 months receive unemployment benefits of 60% (67% in case of parenthood) of their prior gross earnings. The duration of entitlement depends on the duration of the prior employment and on age. For individuals aged less than 50, the maximum duration of entitlement is one year. After this benefit has expired, individuals will only receive a basic payment at subsistence level, which takes into account income and wealth of all household members. See Schmieder and Trenkle (2020) for a more detailed description of the German unemployment insurance system.

²⁵ Note that the majority of our observation period is characterized by very good labor market conditions in Germany that are often described as the "German labor market miracle" (Burda & Hunt, 2011). Illing et al. (2024), who use German administrative data on involuntary job losses occurring between 2002 to 2012, also find a high propensity of swift re-employment.

childcare-related tasks (Beckmannshagen & Sperling, 2024; Samtleben, 2019; Schäper, Schrenker & Wrohlich, 2023).

However, an interesting pattern that emerges throughout our analyses is that women's desired working hours react immediately to their partners' job loss, while actual hours only adjust with a delay of one year. This pattern is particularly pronounced in scenarios where we find slightly larger effects, such as considering only couples where women worked less than full time (see Fig. 3) or couples where the male partner was still unemployed in period 0 (see Fig. 6).

This is consistent with findings by Knaus and Otterbach (2019) and Euwals (2001) who find that adjusting working hours within an existing job is difficult for many employees, in particular for women. In addition, if adverse macroeconomic or regional labor market conditions lead to the male partner's dismissal or plant closure, these conditions may correlate with the female partner's perceived and actual chances of adjusting their working hours or finding a job. In fact, Halla et al. (2020) and Illing et al. (2024) argue that correlated shocks affecting both partners working in similar regions and industries are a potential explanation for the absence of an added worker effect. This idea is closely related to the literature on the so-called discouraged worker effect (see e.g., Benati, 2001; Van Ham, Mulder & Hooimeijer, 2001).

As we generally find very similar responses in women's desired and actual labor supply, we can draw from our analyses that household income shocks due to involuntary job loss do not trigger long-lasting mismatches between desired and actual working hours. At the same time, these mismatches seem to be a pervasive characteristic of the German labor market that is well documented (Beckmannshagen & Schröder, 2022; Knaus & Otterbach, 2019). It is left for future research to systematically discover the drivers of mismatches between desired and actual working hours.

6 Conclusion

In this paper, we conduct an in-depth examination of the adjustments in actual labor supply and stated labor supply preferences of women after their partners suffered an involuntary job loss. In doing so, we shed light on the question of whether the absence of more substantial added worker effects is in line with women's labor supply preferences or whether it is due to an inability to realize their labor supply preferences.

Our event study analysis shows that both actual working hours and desired working hours of women increase marginally after their partners' job loss. Thus, we provide evidence that the small added worker effect which we observe in Germany is consistent with women's labor supply preferences. The absence of larger adjustments in labor supply cannot be explained by labor market frictions preventing women from adjusting working hours according to their changed preferences. Instead, our results indicate that the household income shock caused by the involuntary job loss is only small and often temporary as the majority of affected men find new jobs in the same year in which the job loss occurs. At the same time, in the short run, the German unemployment insurance system is rather generous and offers high replacement rates. The interplay of these two factors—many of the affected men finding

new jobs relatively quickly, and insurance through the tax and transfer system during the unemployment period— may alleviate the pressure on the female partner to substantially adjust labor supply and thus likely presents a reason for the small magnitude of desired and actual added worker effects.

In general, our results imply the persistence of the intra-household division of paid and unpaid work—even if households face exogenous (income) shocks. This also suggests that short-term changes in partners' time availability do not suffice to achieve a more gender equal intra-household division of labor.

Nonetheless, our analyses provide suggestive evidence that immediately after their partner's job loss, women wish to slightly extend their labor supply but are unable to do so in the short run. Against this background, our study can be considered as a starting point for future research on desired added worker effects. For example, future studies could aim to examine whether such effects are more pronounced in local labor markets, industries or periods within the business cycle which are characterized by strong labor market frictions or monopsony power. Furthermore, it would be intriguing to analyze the mismatch between desired and actual labor supply responses to partners' job loss for countries other than Germany with different tax and benefit systems, gender norms, and labor market conditions.

Data Availability The data of the German Socio-Economic Panel are freely available to the research community. Users have to sign a data distribution contract with the SOEP research data center, which can be applied for at: https://www.diw.de/en/diw_01.c.601584.en/data_access.html. Codes that generate all Tables and Figures of the paper based on the SOEP database are available upon request.

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Compliance with ethical standards

Conflict of interest The authors declare no competing interests.

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7 Appendix

7.1 Additional descriptive statistics

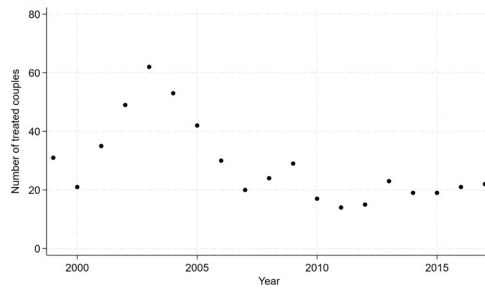


Fig. 7 Number of treated couples by year. Note: Shows yearly number of couples in which the male partner experiences an involuntary job loss. Only includes the first experienced involuntary job loss. Source: SOEP v37.

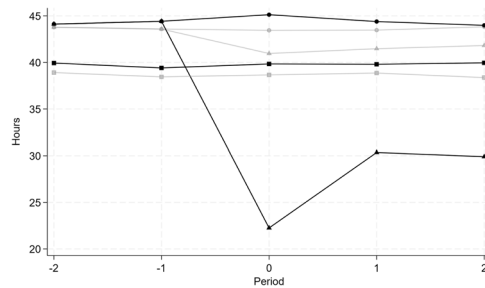


Fig. 8 Hours of displaced men around involuntary job loss. Note: Show actual and desired working hours of men in treatment and control group around the event year. Black (gray) lines and symbols refer to the treatment (control) group. Points (squares) refer to actual (desired) hours only including employed individuals. Triangles refer to actual hours for the full sample counting individuals out of employment as working zero hours. Source: SOEP v37.

Table 2 Descriptive statistics, before and after matching

	Full SOEP	Potentially treated	Matched control	Matched treated	Δ control-treated
Year	2008 (7)	2008 (5)	2007 (5)	2007 (5)	0 (1)
Age (M)	41.46 (13.21)	43.09 (7.29)	42.58 (7.50)	42.29 (7.64)	0.29 (0.56)
Married	0.53 (0.50)	0.87 (0.33)	0.86 (0.35)	0.86 (0.35)	0.00 (1.00)
East	0.22 (0.41)	0.23 (0.42)	0.38 (0.49)	0.38 (0.49)	0.00 (1.00)
Children u3	0.06 (0.23)	0.04 (0.20)	0.04 (0.20)	0.02 (0.15)	0.02 (0.19)
Children 3-5	0.08 (0.27)	0.14 (0.35)	0.11 (0.32)	0.12 (0.33)	-0.01 (0.61)
Children 6-11	0.14 (0.35)	0.30 (0.46)	0.29 (0.45)	0.29 (0.45)	0.00 (0.94)
Children 12-17	0.14 (0.35)	0.28 (0.45)	0.30 (0.46)	0.29 (0.46)	0.01 (0.83)
Need care	0.03 (0.16)	0.02 (0.12)	0.01 (0.11)	0.01 (0.11)	0.00 (0.76)
Sec. educ. (M)	0.66 (0.47)	0.71 (0.46)	0.65 (0.48)	0.63 (0.48)	0.03 (0.37)
Sec. educ. (F)	0.82 (0.39)	0.80 (0.40)	0.79 (0.41)	0.79 (0.41)	0.01 (0.81)
Firm size > 200 (M)	0.67 (0.47)	0.60 (0.49)	0.32 (0.47)	0.28 (0.45)	0.04 (0.24)
Tenure (M)	11.17 (10.57)	14.27 (9.43)	9.11 (7.79)	8.64 (8.95)	0.47 (0.41)
Unemp exp. (M)	0.94 (2.46)	0.26 (0.97)	0.56 (1.67)	0.70 (1.45)	-0.14 (0.18)
Labour inc. (M)	33,827.74 (28,717.53)	53,135.82 (25,519.80)	39,783.73 (24,001.92)	36,753.11 (26,765.83)	3,030.62 (0.07)
Labour inc. (F)	16,884.68 (19,223.45)	25,731.96 (19,300.43)	25,126.12 (18,852.95)	25,173.38 (17,550.29)	-47.26 (0.97)
HH net inc.	43,995.88 (24,647.07)	58,900.89 (22,363.66)	52,958.32 (23,601.78)	50,809.34 (22,675.24)	2,148.98 (0.16)
Actual hours (F)	30.61 (13.11)	28.97 (12.77)	31.98 (13.28)	32.14 (12.99)	-0.16 (0.85)
Desired hours (F)	28.56 (10.13)	27.27 (9.85)	29.89 (9.92)	29.97 (9.88)	-0.08 (0.91)
Observations	210,863	19,543	449	449	0

Note: Table shows means and standard deviations (in parentheses below) of all variables used in the matching procedure for the full SOEP sample, the sample of potentially treated as well as the matched treatment and control groups. Last column shows the difference between the matched control and treatment group. Displayed in parentheses below are p -values for t -tests that the differences are significantly different from zero. The p -values imply the two groups are not significantly different in any of the variables at the 95% significance level. *Source:* SOEP v37.

7.2 Additional regression analyses

7.2.1 Analyses on the extensive margin of labor supply

The difference-in-difference approach applied for extensive margin outcomes follows

$$Y_{i,t} = \beta B_t \times T_i + \nu_i + \tau_t + \varepsilon_{i,t}, \quad (2)$$

where B_t is a dummy variable that is 1 the period after the involuntary job loss, T_i is the respective treatment group dummy, ν_i is an individual fixed effect, τ_t is the year dummy, and $\varepsilon_{i,t}$ is an idiosyncratic error. In this setting the β coefficient of the interaction term between treatment indicator and the pre-/post-shock dummy is the coefficient of interest.

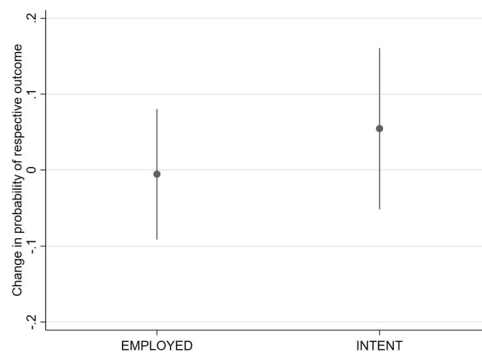


Fig. 9 Extensive margin outcomes. *Note:* Shows results of the difference-in-difference analysis of extensive margin outcomes. Number of individual observations: 362 (181 treated, 181 control units). *Source:* SOEP v37.

7.2.2 Men's labor supply reactions to female partners' involuntary job loss

Our main analyses focus on women's labor supply preferences after their male partner is affected by an involuntary job loss. One of the main reasons for concentrating on women's labor supply preferences is the high share of male workers already working in full-time positions. Thus, there is little scope for male partners to increase labor supply and compensate for lost income due to their partners' job loss. However, for reasons of transparency and comprehensiveness, we also conducted a full analysis based on male labor supply preferences after female partners' involuntary job loss.

Figure 10 shows the results of the main analyses considering households in which the female partner was affected by an involuntary job loss. Panel A shows that the employment rate among affected women drops by about 40% in period 0, while the effect is around -20% in periods 1 and 2. Accordingly, similarly to the findings for men affected by an involuntary job loss (see Fig. 1), large parts of the affected population seem to find a new job rather quickly. Panel B depicts the effect on women's gross labor income. On average, it drops by almost 5000€ in period 0 and

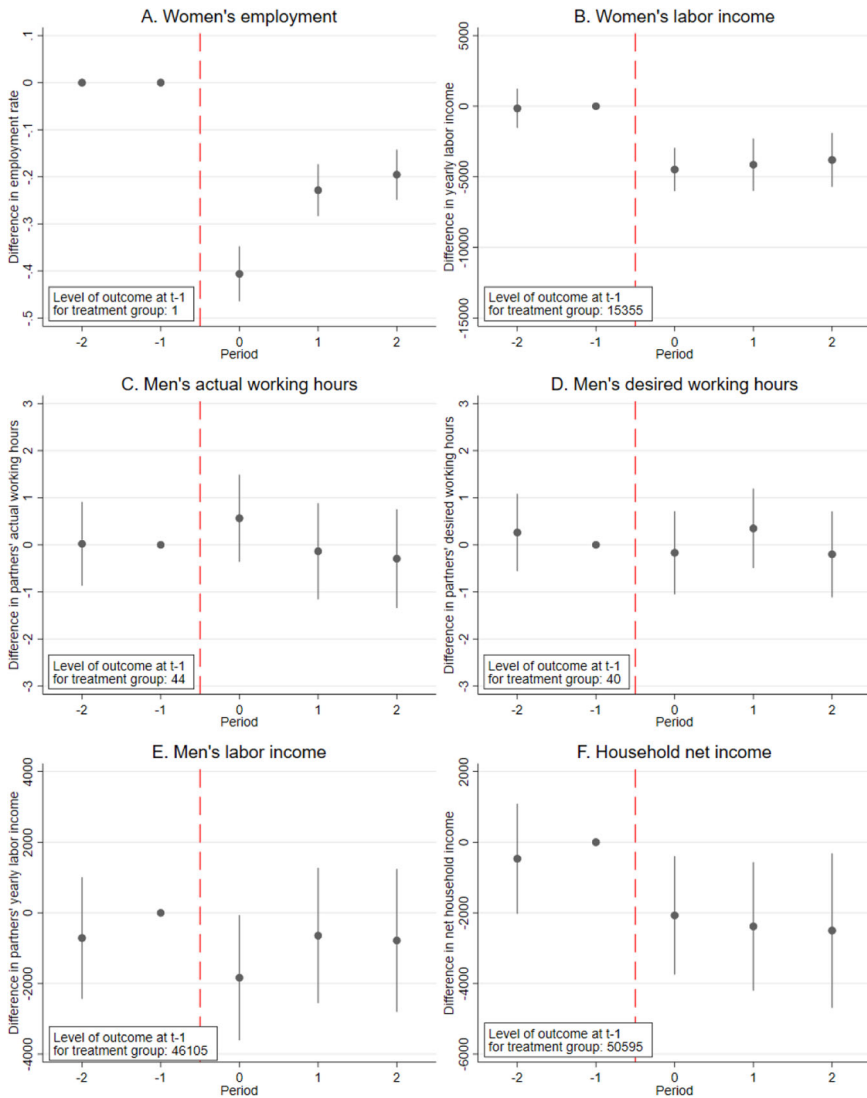


Fig. 10 Main outcomes for men after female partners' job loss. *Note:* Shows period-specific coefficients according to Eq.(1). All incomes are price-adjusted and presented in 2019 Euros. Bars give robust 95% confidence intervals of the respective coefficients. Number of individual observations: 788 (399 treated, 399 control units). *Source:* SOEP v37.

partially recovers in the following periods. In terms of absolute values, the income shock for women is much smaller than for men. However, we also have to consider that women's pre-shock incomes are substantially lower (15,400€ compared to 31,527€ for men). When analysing the male partners' labor supply (Panel C and D), we find no significant effects for actual or desired working hours. Most strikingly, the estimate for desired hours is very precisely zero. Thus, as expected, our results indicate that male labor supply remains unchanged after female partners' involuntary

job loss. As depicted in Panel F, the shock to household net income is small and only significantly different from zero in period 1.

7.2.3 Difference-in-difference estimation for main outcomes

Additionally to the period-specific event study framework in the main analysis, we also execute a standard difference-in-difference estimation, comparing outcomes, $Y_{i,t}$, before and after the job loss occurred. We estimate

$$Y_{i,t} = \beta B_t \times T_i + \nu_i + \tau_t + \varepsilon_{i,t}, \quad (3)$$

where B_t is a dummy variable that is 1 in any post-shock period, T_i is the respective treatment group dummy, ν_i is an individual fixed effect, τ_t is the year dummy, and $\varepsilon_{i,t}$ is an idiosyncratic error. In this setting the β coefficient of the interaction term between treatment indicator and the pre-/post-shock dummy is the coefficient of interest.

As Table 3 shows, we find substantial and statistically significant reductions in men's employment rate and labor income following an involuntary job loss. Actual and desired working hours of their female partners increase in very similar magnitude. However, the confidence interval for actual hours encompasses zero. The point estimate for women's labor income indicates a slight increase, but is not statistically significant. Household net income decreases by about 3000€.

Table 3 Results of Difference-in-Difference estimation, main outcomes

	M emp.	M inc.	F act. hrs.	F des. hrs.	F inc.	HH net inc.
$\hat{\beta}$	-0.33	-8673	0.76	0.78	568	-3170
95% CI	(-0.36 to -0.29)	(-10,394 to -6951)	(-0.06 to 1.58)	(0.05 to 1.51)	(-626 to 1763)	(-4540 to -1,800)

Note: Table shows coefficients and 95% confidence intervals for the interaction term in six separate regression of each outcome variable in a simple diff-in-diff setting. See eq. (3) for the regression equation.

Source: SOEP v37.

7.2.4 Further robustness checks

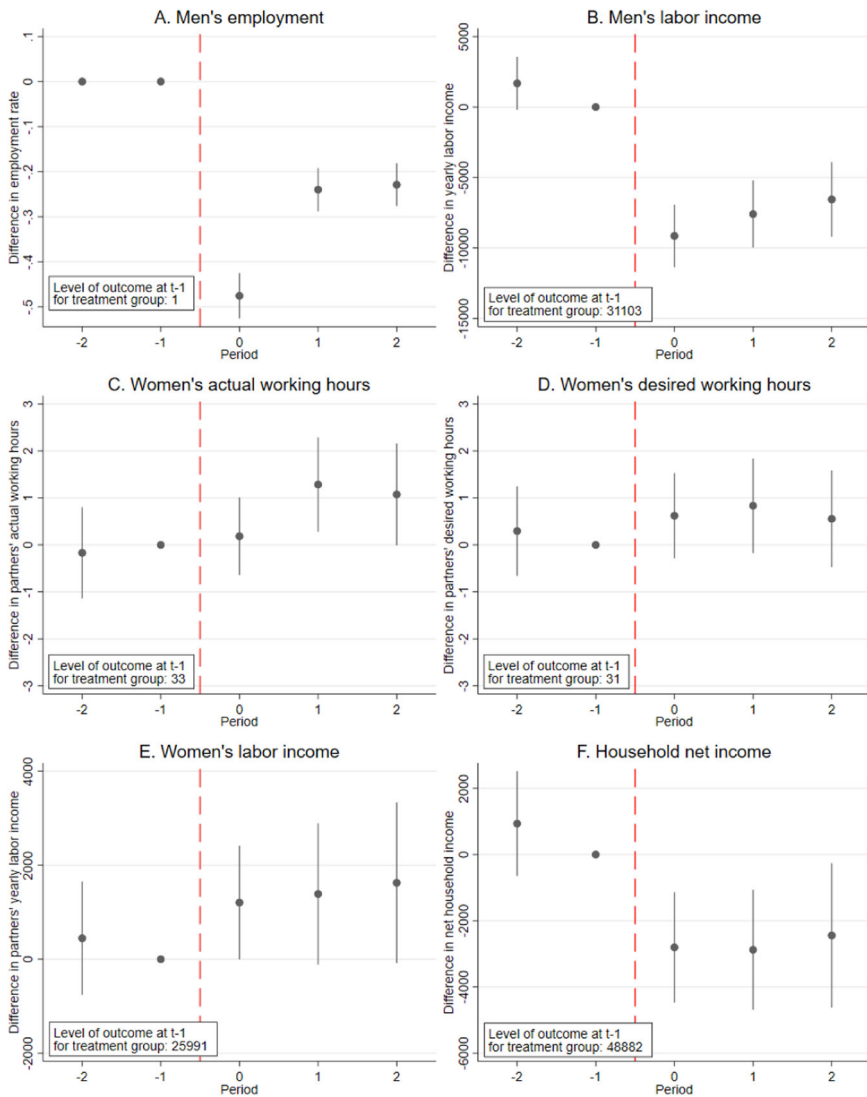


Fig. 11 Main outcomes when considering only the last experienced job loss. *Note:* Shows period-specific coefficients according to Eq.(1). All incomes are price-adjusted and presented in 2019 Euros. Bars give robust 95% confidence intervals of the respective coefficients. Number of individual observations: 900 (450 treated, 450 control units). The number of observations is slightly higher than in the main analysis because one additional couple can be matched successfully when considering the last experienced job loss. *Source:* SOEP v37.

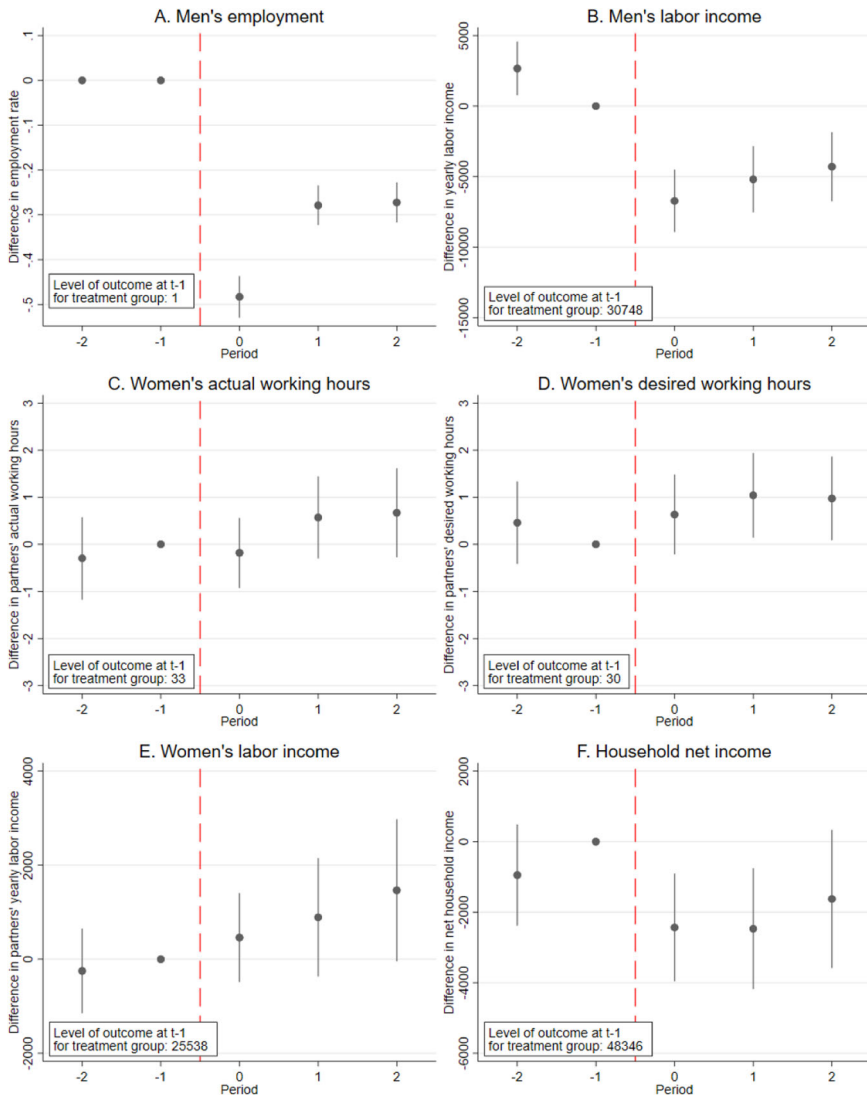


Fig. 12 Estimates based on Mahalanobis distance matching. *Note:* Shows period-specific coefficients according to Eq.(1). All incomes are price-adjusted and presented in 2019 Euros. Bars give robust 95% confidence intervals of the respective coefficients. Number of individual observations: 940 (470 treated, 470 control units). *Source:* SOEP v37.

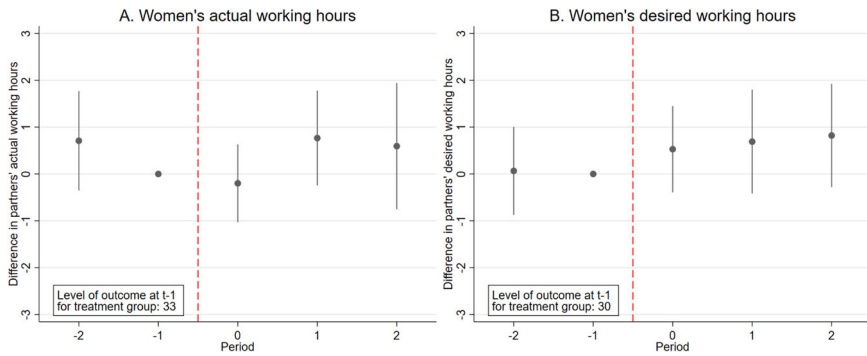


Fig. 13 Results of Callaway and Sant'Anna (2021) estimator. *Note:* Shows period-specific coefficients according to Eq.(1). Bars give robust 95% confidence intervals of the respective coefficients. Number of individual observations: 898 (449 treated, 449 control units). *Source:* SOEP v37.

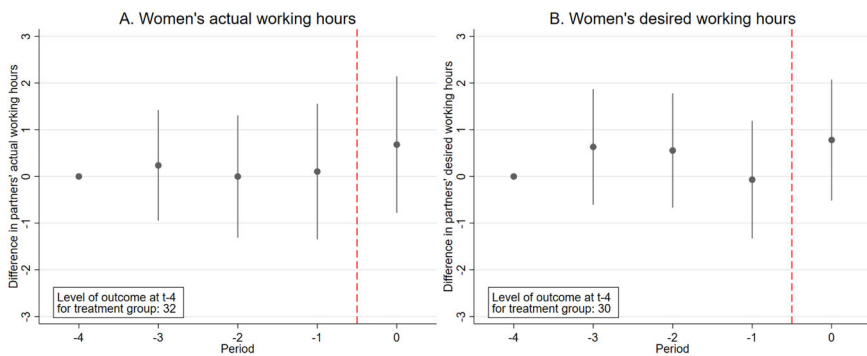


Fig. 14 Extended pre-shock period. *Note:* Shows period-specific coefficients. In contrast to all other event study figures, reference period is t-4. Bars give robust 95% confidence intervals of the respective coefficients. Number of individual observations: 788 (389 treated, 389 control units). Note that the number of observations slightly deviates due to the shift in observation period. *Source:* SOEP v37.

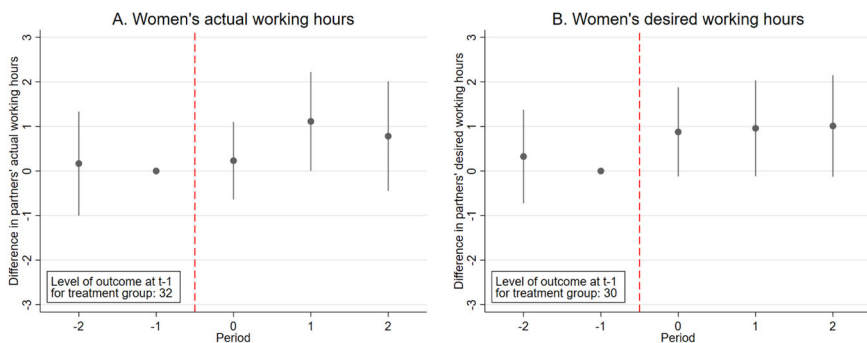


Fig. 15 Results for married women. *Note:* Shows period-specific coefficients according to Eq.(1). Bars give robust 95% confidence intervals of the respective coefficients. Number of individual observations: 770 (385 treated, 385 control units). *Source:* SOEP v37.

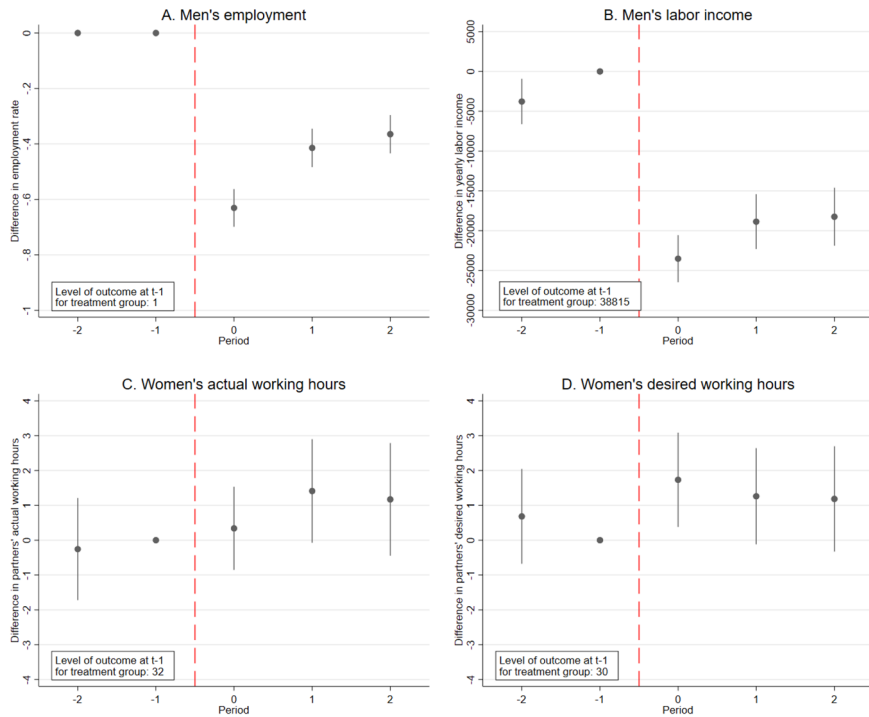


Fig. 16 Results for top half of income loss. *Note:* Shows period-specific coefficients according to Eq.(1) after restricting the sample to only couples in the top half of the distribution of relative income loss from period -1 to 0 . Gross labor income is price-adjusted and presented in 2019 Euros. Bars give robust 95% confidence intervals of the respective coefficients. Number of individual observations: 444 (222 treated, 222 control units). *Source:* SOEP v37.

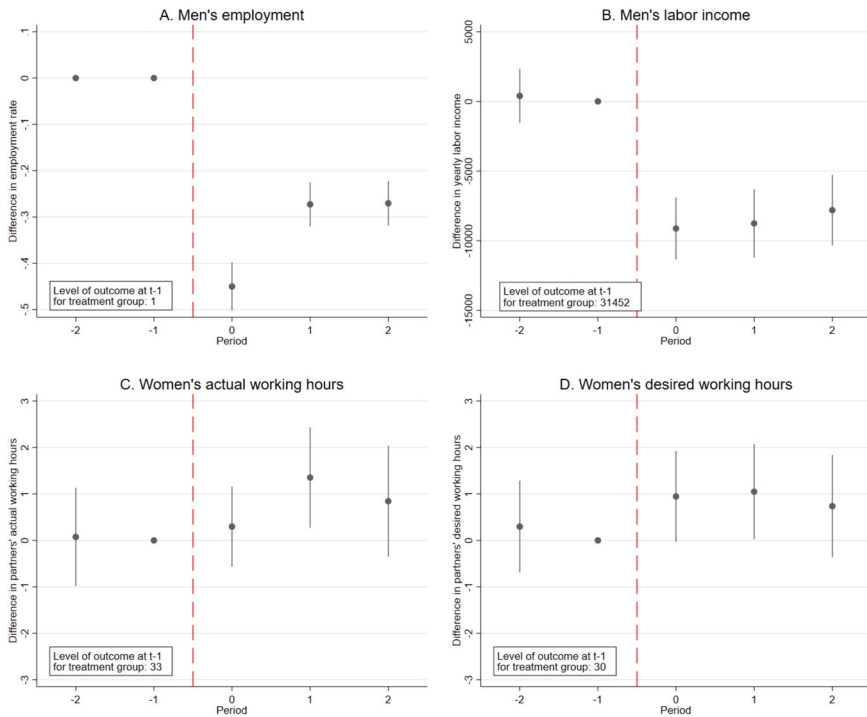


Fig. 17 Results when men were in full-time employment before job loss. *Note:* Shows period-specific coefficients according to Eq.(1) after restricting the sample to only couples in which the male partner works in full-time employment in period -1 . Gross labor income is price-adjusted and presented in 2019 Euros. Bars give robust 95% confidence intervals of the respective coefficients. Number of individual observations: 858 (429 treated, 429 control units). *Source:* SOEP v37.

7.3 Survey questions for key variables

77. If you could choose your own working hours, taking into account that your income would change according to the number of hours:
How many hours would you want to work?
 , hours per week

Fig. 18 Survey question on desired working hours in the SOEP. *Note:* Contains the question on desired working hours from the 2017 questionnaire. The questions was asked in the same way from 1997 onward.

34. Do you intend to obtain (or resume) employment in the future?

No, definitely not..... ☐ ➔ Question 107I

Probably not ☐

Probably ☐

Yes, definitely ☐

Fig. 19 Survey question on intention to return to work in the SOEP. *Note:* Contains the question on the intention to return to work from the 2017 questionnaire. The questions was asked in the same way from 1997 onward.

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